sure reward in its advancement wherever this method might be applied. So much for these objections.

New notations in the calculus of quaternions must needs be invented from time to time. But since they are becoming complex (though far simpler than in cartesian coordinates) as the problems are getting more complicated, it is highly desirable already at this stage of development, to exchange opinions on the selection or adoption of new symbols.

By these and other considerations we have been led to believe that the time has come for those who are interested in vector analysis to come to the fore and join hands. In order to further this purpose, we venture to suggest the establishing of something like an "International Association for Promoting the Calculus of Quaternions." The following would be amongst its principal objects:—

(1) That the members should be informed of the publications of all important papers and works respecting either the theory of quaternions or its applications; and if possible to have these made accessible to them.

(2) That the members should be afforded the means of exchanging opinions on the introduction and adoption of new notations.

In these few lines we have tried to point out the important task of the Association, but shall be obliged for any suggestion or improvement. All we desire is to assure to the calculus the place it deserves, and consequently to see it fully developed in its various aspects by the combined efforts of able mathematicians and physicists. It is almost needless to say that we are only preparing the way; and once the Association has been started, we shall be ready to place it in the hands of persons much more competent than ourselves to further its best interests.

We earnestly hope that all friends will appreciate our endeavours and show us at once some token of approval. We would ask those who are in Europe to communicate with the first of the names below, and those in America with the second.

P. Molenbroek, The Hague, Holland.
Shunkichi Kimura, Yale University, U.S.A.
August 7.

P.S.—It has been suggested by friends interested in this matter to enlarge the scope of the proposed Association so as to include all systems allied to quaternions and to Grassmann's "Ausdehnungslehre." This suggestion we are in full sympathy with. The name of the Association might then be "The International Association for Promoting the Study of Quaternions and Allied Systems of Mathematics." P. M.
September 17. S. K.

Artificial Human Milk.

It is stated in Nature of September 19, that "so far, according to Dr. Backhaus, no satisfactory substitute has been produced in the place of human milk"; and a method is then described by which he has "quite recently" succeeded in supplying the deficiency. It appears to differ little from the process first employed and made known by me in 1854, and afterwards published in my "Experimental Researches" in 1877; except that, in omitting to add the necessary amount of milk-sugar to make up for the deficiency in the cow's milk, Dr. Backhaus fails to obtain an artificial milk closely resembling the human in chemical composition.

My recipe has, since its first publication, been advantageously used in private and hospital practice by the late Prof. W. C. Williamson, by Dr. W. Playfair, and others, but it has probably not come under the notice of Dr. Backhaus.

My process is based on the fact that by the removal of one-third of the casein from cow's milk, and the addition of one-third more milk-sugar, a liquid is obtained which closely approaches human milk in composition. The following is the mode of preparing the milk, and it is so simple that any intelligent mother or nurse can easily carry it out.

"Allow one-third of a pint of new milk to stand for about twelve hours, remove the cream, and add it to two-thirds of a pint of new milk, as fresh from the cow as possible. Into the one-third of a pint of blue milk left after the abstraction of the cream, put a piece of rennet about one inch square. Set the vessel in warm water until the milk is fully curdled, an operation requiring from five to fifteen minutes, according to the activity of the rennet, which should be removed as soon as the curdling commences and put into an egg-cup for use on subsequent occasions, as it may be employed daily for a week or two. Break up the curd repeatedly and carefully separate the whole of the whey, which should then be rapidly heated to boiling in a small tin pan placed over a spirit- or gas-lamp. During the heating, a further quantity of casein separates, and must be removed by straining through muslin. Now dissolve 110 grains of powdered milk-sugar in the hot whey, and mix it with two-thirds of a pint of new milk to which the cream from the other third of a pint was added, as already described. The artificial milk should be used within twelve hours of its preparation; and it is almost needless to add, that all the vessels employed in its manufacture and administration should be kept scrupulously clean."

In this process only one-third of the milk was sterilised; but, in the light of modern bacteriology, it is desirable to sterilise the whole by finally heating it to boiling.

The Yews, Reigate, September 29. E. Frankland.

The Elements of Architecture.

Having been for some weeks out of the way of seeing papers, I have only just seen the review of "Architecture for General Readers" in Nature of August 15. I ought to thank you for devoting so much space to a book which deals rather with art than "nature," and there are one or two criticisms on special points which I think are just, and which will have attention in the second edition of the book. But there are three remarks of the reviewer's on which I should like to have a word.

(1) He refers the reader to Perrot and Chipiez' work on "The Arts of Primitive Greece" for proof of the derivation of the Greek entablature from a wooden origin. In my opinion, Messrs. Perrot and Chipiez prove nothing whatever but their own ingenuity. They argue in a circle. Assuming the probability of a wooden origin for the Greek entablature, they proceed to construct out of their own inner consciousness a series of wooden structures, quite possible but entirely imaginary, in which the origin of all the features of the stone entablature is carefully provided for, and then produce an engraving of the stone (or, rather, marble) entablature to show triumphantly the result which they have been consciously leading up to all the way. You may prove anything on that kind of principle. I do not deny that the Greek entablature appears to be of timber origin. I only say it has not been proved to be so, and I am sure Messrs. Perrot and Chipiez have not proved it.

(2) The reviewer thinks I am captious in objecting to Wren's double cupola at St. Paul's as a sham, and that I might as well object to the vault which hides the interior of the tower over the crossing in a mediæval cathedral. But he misses the main point of my objection, which is that the exterior timber dome of St. Paul's is made to appear, to the eye, to carry a ponderous stone lantern which would, in fact, crush it at once, and which is really the termination of a concealed masonic construction thrusting itself through the timber dome. At Florence and St. Peter's the stone lantern is really carried by the visible dome which appears to carry it; at St. Paul's it is not, and could not be. I consider St. Paul's by far the more beautiful design of the three, but it cannot be denied that it is a constructional falsehood in that respect. (See the block section of it given on p. 99 of the book.)

(3) The reviewer objects that I have denied to Italy any specimen of true Gothic, and yet that Milan is one of the most impressive Gothic interiors in existence. This may be true as to general effect; but the detail of Milan is wretched; and it is by detail that purity of architectural style is chiefly to be judged.

H. Heathcote Statham.

(1) Mr. Statham objects to Perrot and Chipiez' work, on primitive Greece being cited for proof of the derivation of the Greek entablature from a wooden origin.

It seems to me that in this matter possibly the main difference between Mr. Statham and the reviewer lies in the meaning to be attached to the word *proof*. Absolute mathematical proof is seldom to be looked for in archæological or historical descriptions, and we must be often contented with a sufficiently high probability. Taking the word in that sense, it seems to me that the circle in which Perrot and Chipiez are said to argue, cannot be made to re-enter into itself.

Mr. Statham allows that the Greek entablature "appears to be of timber origin." Vitruvius (iv. cap. 2) says distinctly that it

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was so. The remains of primitive architecture in Greece—particularly at Tiryus—show that wood must have entered largely into architectural constructions; amongst other evidences, the traces of wooden door-cases cannot be explained away. Perrot and Chipiez, with whatever amount of fancifulness there may be (and there is no doubt much which is altogether hypothetical) in their restorations, do come legitimately to an explanation of the Doric guttæ both under the triglyphs and beneath the mutules, as typical of the ends of wooden pegs or trenails in timber construction, which is sufficient for the argument in the review, in which there was no intention to approve Perrot and Chipiez' restorations and deductions any further than that.

(2) As to the second objection taken to the review—the remark respecting the cupola of St. Paul's. The remark in the review had reference to the objection that the external outline of the dome was distinct from the internal, and not to the question of support of the lantern; but with reference to the latter point, when the lantern of St. Peter's is quoted as supported by a more legitimate construction than that of St. Paul's, it may be asked: Why the construction of St. Peter's dome, which is absolutely dependent for its safety on the iron chains by which it is hooped together, is preferable to that of St. Paul's, where the lantern has a much securer, and therefore not less legitimate, support in Sir Christopher Wren's cone?

(3) One remark only on the objection raised to the style of Milan Cathedral. The detail is said to be wretched. That it does not conform to the canon of Northern Gothic can be readily conceded, but that the shafts of the magnificent forest of pillars which support the interior are wretchedly designed, and unsuitable to the intended effect, is not so easy to admit.

THE REVIEWER.

Do the Components of Compound Colours in Nature follow a Law of Multiple Proportions?

This question, put by Mr. F. Howard Collins in Nature (p. 438), may be answered in the negative.

In practical work there is no indication of such a law. It is found that the two rays, which together produce a compound natural colour, may be in any proportions; when there is a multiple proportion, and in some cases there must be, it is only as forming part of a series of variations, such as are frequently found within the limits of a single popular colour term. How wide these proportions may be, can be illustrated by comparing them to the varying proportions of two irregular curves towards each other.

The examples of foliage quoted can only be taken as representing individual instances. Variations of climate, age, cultivation, and aspect alter the colour proportions of a given variety of leaf; indeed, such variations are sometimes found in the same leaf.

JOSEPH W. LOVIBOND.

Salisbury, September 23.

In view of the letters, recently printed in Nature, by Mr. H. H. Pillsbury and Mr. Herbert Spencer, it may be well to state that Chevreul published an "Exposé d'un moyen de définir et de nommer les couleur d'après une méthode précise et expérimentale" (Paris, 1861, also *Mem. de l'Acad.* xxxiii.), in which elaborate charts are given showing the colours defined by a decimal system and in ten degrees of saturation.

Recently Prof. W. Hallock, of this College, has painted discs with standard colours, and determined their wave-lengths with the spectroscope. These discs were then used to study 6000 samples of coloured objects, and formulæ were determined for some 500 named colours. These formulæ have been used for defining the names of colours in the new "Standard Dictionary" (Funk and Wagnall's, New York).

J. McKEEN CATTELL.

Columbia College, New York, September 20.

A Problem in Thermodynamics.

It may interest some of your readers to know that the problem in thermodynamics, propounded by Mr. Blass in your number of August 29, has actually been put to the test. I pointed out Mr. Blass's letter to my brother, who is a freezing engineer, and he showed me a copy of the *Zeitschrift für die Gesammelte Kälte-Industrie* (Munich) for August, in which an account is given of a machine on exactly the principle Mr. Blass suggests, by which Herr Linde has succeeded in liquefying air. It would appear, therefore, that the "theoretical minimum of temperature produced at C" would be determined by the point of liquefaction of the gas employed; with a perfect unliquefiable gas it would, I suppose, theoretically, be absolute zero.

EDWARD T. DIXON.

Cambridge, September 22.

THE NEW MINERAL GASES.

OUR knowledge of the spectra and other conditionings of the new mineral gases has received an important addition in the communication from Drs. Runge and Paschen which appeared in last week's Nature. The employment of exposures extending over seven hours has given a considerable extension in the number of lines, and the bolometer has been called in to investigate lines in the infra-red; better still, they have employed well-practised hands in searching for series of lines. Operating, by chemical means, upon a crystal of clèveite free from any other mineral, they have obtained a product so pure that from these series there are no outstanding lines. Very great weight, therefore, must be attached to their conclusions, and there are several points of contact with the work upon which I have been engaged from a slightly different stand-point since last April, when Prof. Ramsay made his fortunate discovery of a terrestrial source of helium.

I will touch upon some of these points *seriatim*.

In the first place, there has never been the slightest doubt in my mind that it was a question of gases and not of a gas. The spectroscopic evidence in the laboratory alone was complete, and the case was greatly strengthened when the behaviour of the various lines in the sun and stars was also brought into evidence. Drs. Runge and Paschen also declare that the gas given off even by a pure crystal of clèveite is not simple, but consists of two constituents. To the one containing the line D_3, which I discovered in 1868, the name helium remains; the other for the present, we may call "gas X." The chief lines of these two constituents are as follows, according to Runge and Paschen:

Helium.	Gas X.
5876	6678
4713	5048
4472	5016
4026	4922
3889	

Last May I wrote as follows[1]:—

"The preliminary reconnaissance suggests that the gas obtained from bröggerite, by my method, is one of complex origin.

"I now proceed to show that the same conclusion holds good for the gases obtained by Profs. Ramsay and Clève from clèveite.

"For this purpose, as the final measures of the lines of the gas as obtained from clèveite by Profs. Ramsay and Clève have not yet been published, I take those given by Crookes, and Clève, as observed by Thalén.

"The most definite and striking result so far obtained is that in the spectra of the minerals giving the yellow line I have so far examined, I have never once seen the lines recorded by Crookes and Thalén in the blue. This demonstrates that the gas obtained from certain specimens of clèveite by chemical methods is vastly different from that obtained by my method from certain specimens of bröggerite, and since from the point of view of the blue lines, the spectrum of the gas obtained from clèveite is more complex than that of bröggerite, the gas itself cannot be more simple.

"Even the blue lines themselves, instead of appearing

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[1] *Proc.* Roy. Soc., vol. lviii. p. 114.

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